THE JOURNAL
OF THE
ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 147 MARCH 1923 VOL. XXVII

NOTICES

Election of Members

The following members were elected at a meeting of the Council held on February 20th:—

Honorary Fellow.—J. L. Pritchard.

Associate Fellows.—F. W. Johnson, M. A. Zahra.

Students.—G. A. Fowkes, C. T. Travers.

Foreign Member.—A. R. Stevenson.

Wilbur Wright Lecture

Dr. Joseph S. Ames, of the American National Advisory Committee for Aeronautics, has accepted the Council's invitation to read the Eleventh Wilbur Wright Lecture in the Theatre of the Royal Society of Arts, John Street, Adelphi, at 5.30 p.m., on Thursday, May 31st. He has selected as the subject of his address, "The Relationship between Aeronautical Research and Aircraft Design."

Donations

The Council desire to acknowledge the following donations:—

"Planes and Personalities," by Cunningham Reid.

"Edward Teschmaker Busk," by M.B. These two books, formerly in the possession of the late Sir Walter Raleigh, were presented by Captain J. Morris, A.F.R.Aë.S.

"Notes on the Life of Frederick Marriott," presented by his grandson, Sir Cyril Kirkpatrick, M.Inst.C.E.

"In Full Flight," by E. Vine Hall, presented by the Author.

Lantern slides from Major Cleghorn, C. R. Catesby and C. T. Travers.

Annual General Meeting

The Annual General Meeting of voting members of the Society will be held in the Library at 5.0 p.m., on Tuesday, March 27th. The Council's Annual Report and Statement of Accounts for the year 1922 will be found on a later page.

Students' Section

A visit to the works of Messrs. S. Smith and Sons, instrument makers, Edgware Road, Cricklewood, N.W., has been arranged for Saturday, March 17th. Students wishing to take advantage of this opportunity should meet at Messrs. Smith's works at 9.40 a.m.

Further visits have been arranged as follows, of which details will be announced later :—

Thursday, April 19th.—Marconi Company, Chelmsford.

Wednesday, April 25th.—Napier Engine Company, Acton.

Saturday, May 5th.—Vickers Limited, Weybridge.

Wednesday, May 30th.—Fairey Aviation Company, Hayes, Middlesex.

Forthcoming Arrangements

March	1, 5.30 p.m.	Royal Society of Arts. Major F. M. Green, O.B.E., Fellow, "Air Travel with Special Reference to the Helicopter."
,,	8, 7.0 p.m.	*Students' Section.*—Society's Library. Mr. J. D. Campbell, "Air Transport."
,,	15, 5.30 p.m.	Royal Society of Arts. Professor B. Melvill Jones, A.F.C., Associate Fellow, "The Control of Aeroplanes at Slow Speeds."
,,	20, 5.0 p.m.	Council Meeting.
,,	22, 7.0 p.m.	*Students' Section.*—Society's Library. Mr. S. H. Evans, "The Variable Camber Wing."
,,	27, 5.0 p.m.	ANNUAL GENERAL MEETING. Society's Library.

W. LOCKWOOD MARSH, *Secretary.*

ROYAL AERONAUTICAL SOCIETY

58TH ANNUAL REPORT OF THE COUNCIL, 1922-1923

Council

Chairman.—Professor L. Bairstow, C.B.E., F.R.S.

Vice-Chairman.—Lieutenant-Colonel M. O'Gorman, C.B., D.Sc.

Brigadier-General R. K. Bagnall-Wild, C.M.G., C.B.E., Mr. Griffith Brewer, Wing Commander T. R. Cave-Browne-Cave, C.B.E., R.A.F., Sir Mackenzie Chalmers, K.C.B., C.S.I., Mr. H. P. Folland, Sir Robert Hadfield, Bart., Captain G. de Havilland, A.F.C., Squadron Leader R. M. Hill, M.C., A.F.C., Professor C. F. Jenkin, C.B.E., Professor B. Melvill Jones, A.F.C., Lieutenant-Colonel J. T. C. Moore-Brabazon, M.C., M.P., Mr. J. D. North, Lieutenant-Colonel A. Ogilvie, C.B.E., Professor A. J. Sutton Pippard, D.Sc., Colonel The Master of Sempill, A.F.C., Major R. V. Southwell, Lieutenant-Colonel H. T. Tizard, A.F.C., Major H. E. Wimperis, O.B.E.

Honorary Treasurer.—Mr. A. E. Turner.

Arrangements have been made with the Committee of the International Air Congress, which is to be held in London from June 25th to 30th this year, that the organisation of the Congress shall be undertaken by the Society. A grant, which it is believed will cover the additional expenditure, has been made to the Society for the purpose.

It is felt that considerable progress has been made during the period under review towards strengthening the scientific and technical aspects of the Society's activities. The lectures read have been such to effect this end, and the holding of the first examination for Associate Fellowship has also emphasised this tendency.

Library

In July last the Secretary called the attention of the Council to a number of valuable early works on aeronautics which had been collected by Messrs. Maggs Brothers, and were on the point of being offered by them to an American collector unless they were previously sold in this country. The Council therefore decided to approach the members of the Carnegie United Kingdom Trust Fund, with the result that in November intimation was received that the Carnegie Trustees were prepared to make a substantial grant for this purpose. The Secretary spent some days going through the volumes concerned (which numbered some 1,000), and selected from them all those of historical interest, numbering about 250, which were not already in the Society's Library. This acquisition has aroused great interest in literary circles both in England and abroad. An article on it which appeared in the "Times" Literary Supplement is reprinted in this issue.

Membership

The membership numbered 886 on January 1st, 1923, compared with 879 at the same date last year. All subscriptions owing prior to January 1st, 1922, have now been written off, and 50 per cent. of those still outstanding for the year 1922. The Council follow the practice of stopping the supply of the Journal to all Members who have not paid their current subscriptions by June 30th in each year, and removing from the rolls the names of those whose subscriptions have not been received prior to December 31st.

Silver Medal

Following upon the revival of the old practice of awarding the Society's Silver Medal to the author of the paper deemed by the Council to be the best published in the Journal during each year, the silver medal for the year 1921 was awarded to Mr. H. R. Ricardo for his paper entitled "Some Possible Lines of Development in Aircraft Engines."

R38 Memorial Research Fund

It has been decided to devote some of the money raised for this Fund, which at present totals £1,264 19s. 11d., to the placing of a tablet in memory of those lost in R.38 on the wall of the Library, for which a design by Mr. Paul Cooper has been accepted by the Council. In addition to this a prize of 25 guineas is to be awarded annually for a paper on some technical aspect of aeronautics, preference being given to papers dealing with airships. For the first year of this award, the list for which closed on December 31st last, 12 entries have been received, including entries from Germany, U.S.A., and Italy. All papers so far presented deal with airship problems.

Associate Fellowship Examination

The first examination for Associate Fellowship was held in the Library on Tuesday, September 26th. Most of the candidates offered "Aerodynamics" and "Strength of Materials and Theory of Structures" as their subjects, though the papers set on "Heat Engines" and "Meteorology and Navigation" were also taken. All the papers set were published in the issue of the Journal for November, 1922, as specimens for future candidates.

Technical Discussion

It was suggested to the Council during the year that opportunities should be afforded to members for more detailed discussion of problems in aeronautics than is possible at the ordinary meetings of the Society during the lecture programme. Mr. H. Glauert therefore presented a paper on "Theoretical Relationships for the Lift and Drag of an Aerofoil Structure," which was discussed at well-attended meetings in the Library on November 30th and January 10th and is still continuing. The paper and discussion will ultimately be published in the Journal.

Journal

Commencing with the issue of January, 1923, the title of the Journal has been changed to "The Journal of the Royal Aeronautical Society" in order to identify it more definitely as the medium for the publication of the Society's proceedings.

An interesting letter in this connection has recently been received by the Secretary from Major Baden-Powell, who was for some years President and for a time acted as Honorary Secretary of the Society, recalling the fact that the Journal was originally started in 1897 as a private venture of a group of members, and only later (apparently in 1909) became actually the official publication of the Society.

The Council are able to record that the circulation of the Journal among persons outside the membership has very considerably increased during the last twelve months. The number of annual subscriptions have increased from 81 to 136 in that period, and the casual sales show a similar improvement. They are glad again to record their appreciation of the generous help of Mr. J. L. Pritchard in acting as editor and remitting to the Society as a donation his fees. Owing to his efforts the interest and value of the Journal continues to increase, and they desire to congratulate both him and the Secretary on the results shown by the above figures.

Students' Section

In connection with Students' meetings, it will be remembered that the Council announced last year that the Pilcher Memorial Prize for Students would be awarded annually to the Student author of the best paper inaugurating discussion at these meetings.

The first award was made to Mr. S. H. Evans for his paper on "Some Notes on Commercial Aircraft," and books to the value of £5 were accordingly presented to him.

At the first meeting of the present session on October 12th, 1922, Mr. T. C. Sharwood was elected Honorary Secretary in succession to Mr. S. H. Evans.

Since the last report the following visits have been arranged for Students :—

1922.

May 6.—De Havilland Aircraft Company.

,, 31.—Royal Aircraft Establishment.

June 10.—National Physical Laboratory.

1923.

Jan. 27.—London Terminal Aerodrome, Croydon.

The full programme of meetings for the current session is as follows :—

1922.

Nov. 9.—"Airships," H. C. Brown.

,, 23.—"Some Practical Points in Aero Engine Design and Construction," G. R. Irvine.

Dec. 14.—"The Navigation of Aircraft," A. P. Rowe.

1923.

Jan. 25.—"Discussion on English and German Methods of Estimating Aeroplane Performance," F. Radcliffe.

Feb. 8.—"Experimental Methods in Aerodynamics," W. L. Le Page.

,, 22.—"The High Lift Wing," T. A. Kirkup.

Mar. 8.—"Air Transport," J. D. Campbell.

,, 22.—(Title unknown), S. H. Evans.

The lantern has now been repaired and a screen fixed in the Library so that slides may be shown at Students' meetings.

(*Continued on page* 88.)

AERIAL
(The Royal

Balance Sheet,

	£ s. d.	£ s. d.
To *Nominal Capital*—		
Divided into 20 Shares of 1/- each and 999 Shares of £1 each	1000 0 0	
,, *Capital Issued and Called Up*—		
19 Shares of 1/- each		19 0
,, *Sundry Creditors*		1141 0 1
,, *Subscriptions Received in Advance*		29 7 0
,, *Carnegie U.K. Trust*—		
Grant for the Purchase of Books expended during the year 1922 per contra £355 13s. 3d.		432 0 0
,, *Reserve Fund*—		
Entrance Fees and Life Compositions of present Members, as at 31st December, 1921	2615 6 0	
Receipts for twelve months to 31st December, 1922 ...	35 14 0	
	2651 0 0	
Deduct Income and Expenditure Account Deficiency at 31st December, 1921 ... 1406 2 0		
Add Surplus of Expenditure over Income for year to 31st December, 1922 ... ... 278 8 8		
	1684 10 8	
		966 9 4
		£2569 15 5

Income and Expenditure Account

	£ s. d.
To Office Rental, Lighting and Insurance	327 16 8
,, Salaries	1135 17 8
,, Printing, Stationery, etc.	69 8 6
,, Postages and Messengers	85 0 6
,, Library Expenses	36 18 10
,, Office Expenses	49 11 6
,, Exhibitions and General Meetings	77 4 10
,, Journals, Pamphlets, etc.	408 15 11
,, Audit Fee	15 15 0
,, Subscriptions Written Off	170 12 10
,, Donations	9 0 0
,, Income Tax	10 10 0
,, Examination Expenses	2 2 7
,, Honorarium to Editor of JOURNAL	10 10 0
	£2409 4 10

SCIENCE, LIMITED.

Aeronautical Society).

31st December, 1922.

	£ s. d.	£ s. d.
By *Office Furniture, Printed Books, Bindings, Stationery, Old Prints, etc.*, as at 31st December, 1921	305 16 8	
Add Purchased during Year—		
Books 355 13 3		
Furniture 25 0 0		
	380 13 3	
		686 9 11
„ *Stock of* JOURNALS, *Etc.*		368 0 5
„ *Stock of Stationery*		10 0 0
„ *Sundry Debtors including Subscriptions owing*		286 19 0
„ *Investments at Cost*—		
£100 5 per cent. National War Bond	100 0 0	
£783 6s. 0d. 5 per cent. War Loan Inscribed Stock, 1929/47	725 13 6	
		825 13 6
„ *Cash at Bank*		385 0 5
„ *Cash in Hand*		7 12 2
		£2569 15 5

We report to the Shareholders that we have examined the books of the Society and have obtained all the information and explanations we have required. We are not in a position to judge of the value put upon the outstanding subscriptions. Subject to this remark, we are of opinion that such Balance sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given us and as shown by the books of the Society.

3, FREDERICK PLACE,
OLD JEWRY, E.C.2.
26th February, 1923.

(Signed) PRICE, WATERHOUSE & CO.

for the Year Ending 31st December, 1922.

	£ s. d.
By Annual Subscriptions	2067 16 1
„ Interest on Investments and Deposit	47 5 1
„ Thesis Fee	5 5 0
„ Donation from Mr. J. L. Pritchard	10 10 0
„ Balance, being Surplus of Expenditure over Income	278 8 8
	£2409 4 10

Lectures

The Tenth Wilbur Wright Memorial Lecture was read on June 15th, 1922, by Lieutenant-Colonel A. Ogilvie, who read a paper on "Some Aspects of Aeronautical Research."

The following is the full programme of lectures for the present Session, which is the 58th since the Society's foundation :—

1922.

Oct. 5.—Professor L. Bairstow, "The Work of S. P. Langley."
,, 19.—Mr. J. D. North, "The Metal Construction of Aeroplanes."
Nov. 2.—Major A. R. Low, "A Review of Airscrew and Helicopter Theory with Aeroplane Analogies."
,, 16.—Mr. R. McKinnon Wood, "The Co-Relation of Model and Full-Scale Work."
Dec. 7.—Professor C. F. Jenkin, "Fatigue in Metals."

1923.

Jan. 4.—Herr Hugo Junkers, "Metal Aeroplanes."
,, 11.—R. A. Frazer (Juvenile Lecture), "Testing Model Aeroplanes."
,, 18.—Major J. D. Rennie, "Flying Boats."
Feb. 1.—Mr. G. S. Baker, "Ten Years Testing of Model Seaplanes."
,, 15.—Wing Comdr. T. R. Cave-Browne-Cave, "The Practical Aspect of Seaplanes."
Mar. 1.—Major F. M. Green, "Helicopters."
,, 15.—Professor B. Melvill Jones, "The Control of Aeroplanes at Slow Speeds."

Honorary Officials

The cordial thanks of the Society are due to Mr. B. Woodward, the Honorary Solicitor, who has continued to advise on legal matters; and to Mr. A. E. Turner, who, as Honorary Treasurer, has given invaluable assistance in connection with the financial affairs.

Staff

The Council have pleasure in recording their great appreciation of the services and devotion of the staff during the year.

L. BAIRSTOW, *Chairman.*

PROCEEDINGS

FIFTH MEETING, 58TH SESSION

A meeting of the Society was held at the Royal Society of Arts, John Street, Adelphi, London, on Thursday, December 7th, 1922, the Chairman, Professor Bairstow, in the chair.

The CHAIRMAN, in opening the proceedings, said it was his pleasure to introduce to the meeting Prof. C. F. Jenkin, or rather, Prof. Jenkin and what was a new phase of an important subject. He did not quite know how old the idea of fatigue was; in an indefinite form it might be a century old. The idea of the fatigue of metals probably existed before the middle of last century, when Wöhler gave quantitative form to the property. The experiments of Wöhler, which taught us not only to talk of fatigue, but to estimate it, made the first very distinct mark in the history of the subject. The name of Bauschinger was generally associated with the next step—Bauschinger discussed the relation between fatigue and elasticity, and propounded the general hypothesis that fatigue happened when the limits of elasticity of a material were exceeded. Difficulty had been found in defining elasticity, but, in spite of that, Bauschinger's work formed, perhaps what might be called the second definite mark in the understanding of fatigue. That evening the meeting was to hear from Prof. Jenkin what he (Prof. Bairstow) believed was destined to be a third definite mark. Not only had the fatigue limit been identified with the elasticity in Prof. Jenkin's work, but he had gone one stage further in the formulation of a theory, and had deduced the complicated results which had been observed in fatigue from certain very simple initial hypotheses. Still further, his hypotheses, having accounted for known facts, had led Prof. Jenkin to make predictions which were coming true. His paper was an excellent example of the importance of theory in practice. It was quite clear that, as a result of the production of this theory of fatigue, progress in experiment and in application would be greatly accelerated. He then called upon Prof. Jenkin to read his paper.

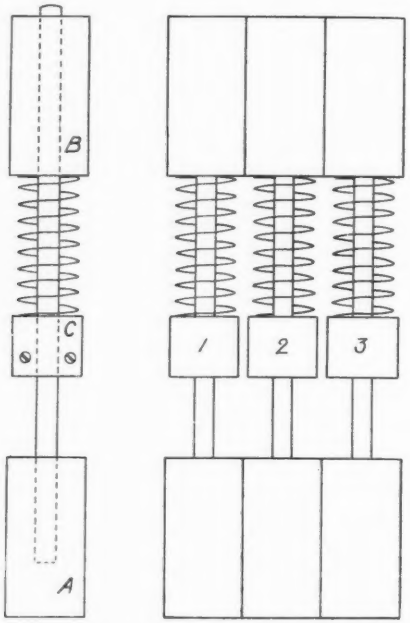
FATIGUE IN METALS

About a year ago I was asked to read this paper. About six months ago I wrote a paper, knowing that I should be very busy this autumn, and made a model to illustrate a small point in it. But as I played with the model to learn how to use it, it grew too strong for me and took command, and for the last six months I have been its obedient slave—for the model explained the whole of my subject—Fatigue. The model destroyed my first paper, and I have been obliged to write a new one in haste, putting it off from day to day as the model taught me new things. The written paper has suffered, but I hope you will find my account of the model more interesting than the tentative theories which filled the old paper.

But in any case, I can speak of nothing else.

A simple form of model is shown in Fig. 1; it is made up of three or more "units" fastened together top and bottom. The units are all similar; each is made up of three pieces—a block A with a rod fixed in it, a block B sliding freely on the rod, and a block C sliding on the rod stiffly, the friction being

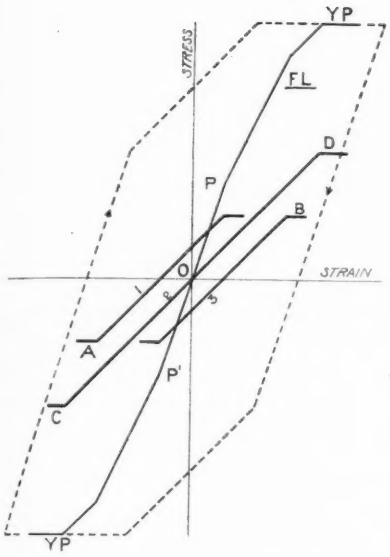
adjustable. Blocks B and C are connected by a spring. The model may be constructed in many other ways; the only requirements are (1) that it shall be elastic up to a point and then slip with solid friction; (2) that all the units shall not slip at once as the load on the model is increased. This latter requirement may be met in two ways—either by making the friction different in the different units, or by making the springs of different strengths; the former method is the simplest and is used in all the models I shall speak of to-day.



"THE ENGINEER" FIG. 1. SWAIN Sc.

The model may be adjusted so as to have different properties by varying the friction in the different units and also by pushing up or pulling down some of the blocks C, so as to give the units initial compression or tension; the model will set itself so that the compression and tension forces balance.

Let us choose a three-unit model for the first test and give No. 2 unit twice the friction of the other two, and give No. 1 unit a small tension, and No. 3 unit an equal compression, so that the model will still be in equilibrium. Let us imagine that the model is put in a static testing machine with an extensometer attached and tested in tension. To show what will happen I will extend the model by hand. At first it stretches elastically, then No. 1 slips, and later No. 3 slips, and finally No. 2 slips. No larger force can be applied. The load/extension diagram given by the extensometer would have form shown in Fig. 2. This figure is easily constructed; the three lines at 45 degrees are the load/extension lines for the three equal springs. They slope up to the points at which they slip and then are horizontal. The graph for the whole model is found by adding the co-ordinates of all three. If the model were made up of more units the graph would be less angular. P is the Limit of Proportionality (Elastic Limit), Y.P. is the Yield Point. F.L. is the Fatigue Limit; how this



"THE ENGINEER" FIG. 2

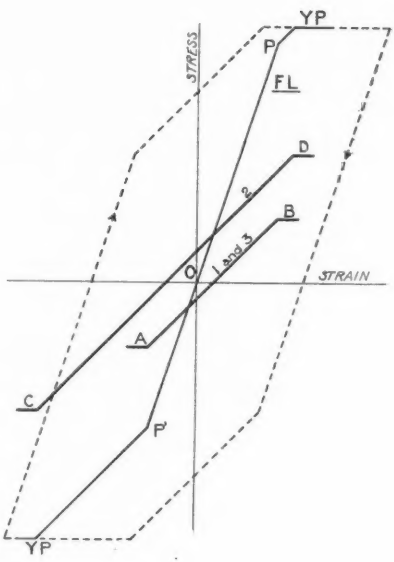


FIG. 4.

is found I shall explain later. Now compare this with Fig. 3, which is a load/strain graph for hardened (but untempered) 100-ton steel.

Next let us set the model differently. Give No. 2 unit a certain tension and Nos. 1 and 3 a compression of half that amount. The load/strain graph is shown in Fig. 4. Compare this with Fig. 5, which is the load/strain graph for a well-tempered steel.

Next let us take a model of ten units and give eight of the units nearly equal friction and the remaining two slightly less. As I pull this model, the

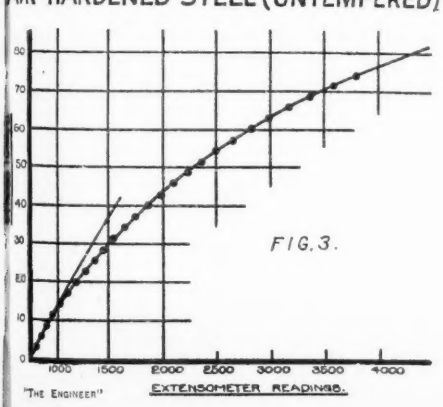


FIG. 3.

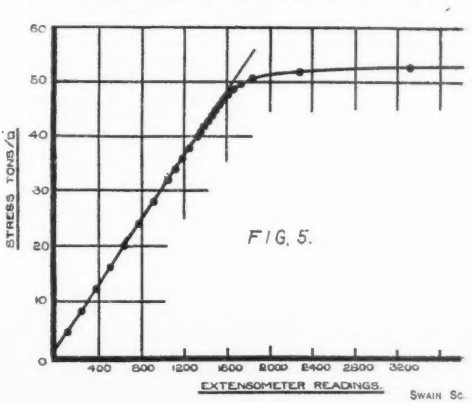


FIG. 5.

weakest unit slips first, then the next. As each slips, more load is thrown on the rest, and when the first of the eight nearly equal units slips the extra load will cause the next to slip at once, and they will all slip—Jack-run-for-mustard. The graph for this model is shown in Fig. 6. Compare this with Dalby's photographic record for mild steel, Fig. 7. In this explanation I have assumed that

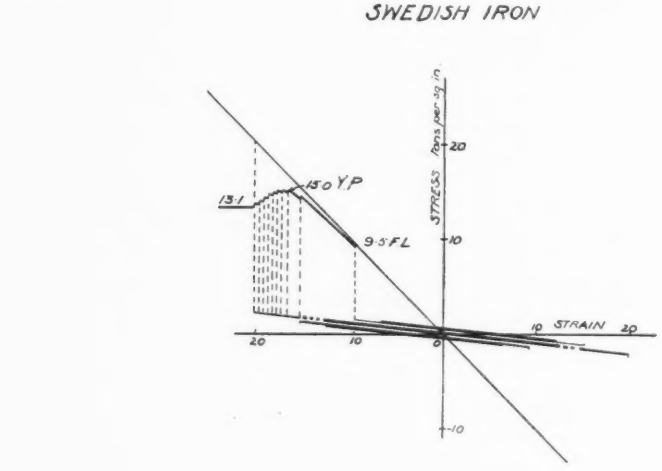


FIG. 6.

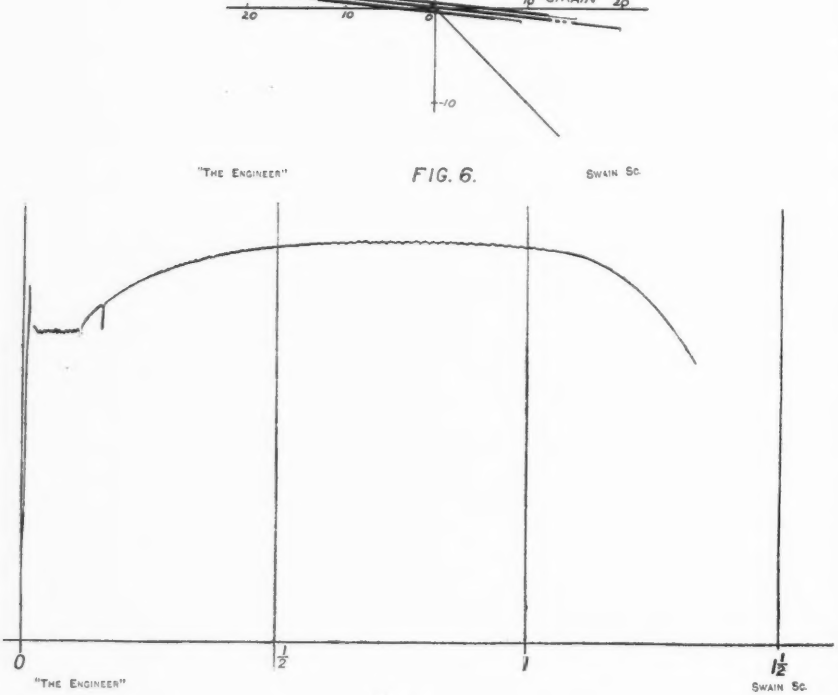


FIG. 7.

when the unit slips it becomes weaker and so throws more load on the rest. I return to this point later.

But there is another very pretty point to notice. Watch the model closely; the units tend to move in little jerks, not quite smoothly, owing to the difference

between the static friction and the sliding friction. These little jerks appear in Dalby's photograph of the steel.

If we compress model No. 2, we shall get the lower curve in Fig. 4. Note that the elastic limits are different in compression and tension. If we carry the test on these models round a cycle we shall get the hysteresis loops shown in Figs. 2 and 4. These have the typical shape of metal hysteresis loops, curved rising lines and straight falling lines. The model will thus represent any of the typical steels very closely—up to the yield point. I have not made it go higher for reasons I shall explain later.

Now let us see what it does when tested under alternating loads. To start with, apply a large alternating strain to the three-unit model. All the units slip backwards and forwards, but as the amplitude of the motion is reduced, first one, then another, and finally all the units cease slipping. The amplitude now is the largest which the weakest unit will stand without slipping. The motion is elastic and, as Bauschinger first stated, the metal will stand this motion indefinitely. We have found that the fatigue limit. When the model comes to rest, the units are all stress-free; they are all in what may be called the central position. The fatigue limit points in Figs. 2 and 4 were found in this way.

The truth of Bauschinger's theorem—the identity of elastic range and fatigue range—has been confirmed by Bairstow (Phil. Trans. Roy. Soc., Vol. 210). Also by Gough ("Engineer," August 12, 1921). Gough invented a test which has turned out to be of the greatest value. A mirror is fixed on the end of the Wöhler test piece and adjusted to run truly in a plane perpendicular to the axis of rotation. A spot of light is reflected by this mirror on to a scale, and as the specimen is loaded this spot moves down the scale. If the deflections are plotted against the load, the graph is a straight line as long as the metal is elastic; thus the limit of proportionality in Gough's rotating test indicates the fatigue limit. The accuracy of this test has been repeatedly confirmed both by Gough and Lea for many metals. This test gives a method of finding the fatigue limit in a few minutes on a single test piece.

Let Gough's test be applied to a copper test piece. Under static test copper is not elastic for even the smallest load; the stress-strain curve bends off from the origin without any straight portion. The model representing copper has the maximum possible initial stresses in the units, so that one begins to slip the moment any load is applied. Applying the alternating load to the model the units will slip into central positions and we shall find a fatigue range as before. But this test will leave the units in a stress-free condition, and we ought, therefore, after the test to find the copper elastic. This remarkable result was found experimentally by Gough, and at my suggestion he repeated the test, raising the range gradually first to one-quarter, then to one-half, then to three-quarters of the full range. In this way the elastic limit of the copper was found to be raised first to one-quarter, then one-half, then to three-quarters of the full range. Thus the experiment exactly confirmed the prophecy based on the model.

The same experiment is now being tried on hardened 100-ton steel. The stress-strain curve (Fig. 3) we have already seen bends off almost from the origin. By applying an alternating stress the steel should be made elastic up to the fatigue limit, according to my theory.

The fatigue limit we have found is for equal tension and compression loads, but there is an infinite series of fatigue limits for unequal loads. How can their magnitudes be found? Suppose that an alternating strain were applied to the model unsymmetrically, so that the tension were greater than the compression; this might be increased till the weakest unit began to slip backwards and forwards; when that occurred we should have reached the fatigue limits for these particular unequal loads, and the range is the same as before. We may go on making such tests, making the motion more and more unsymmetrical, and the range will

remain the same till a new limiting condition intervenes, viz., that the maximum tensile stress must not exceed the ultimate strength of the test piece. This condition reduces the range which can be applied. The graph representing the range of stress plotted against the inferior stress will therefore consist of two straight lines—one horizontal at the range for the weakest unit and the other at 45 degrees through the ultimate strength. See Fig. 8. This graph, according to the text books, should be Gerber's Parabola (shown in the figure). The model appears to fail here. But compare the graph with the actual results shown in Fig. 9 (Bairstow's paper, Fig. 7). The model is clearly right and Gerber wrong.

Before considering further properties of the model, let us see whether there is anything in the metal which can behave as the units in the model behave. The crystals are obviously the units. Ewing and Rosenhain* have shown that

SWEDISH IRON

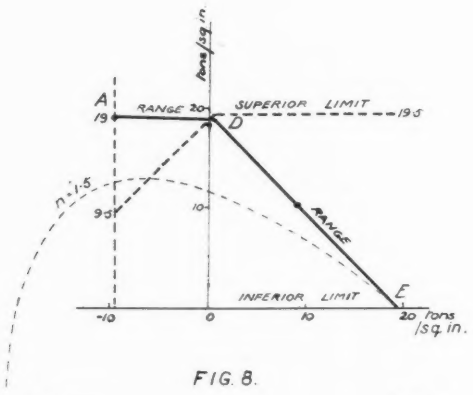


FIG. 8.

"THE ENGINEER" SWAIN SC.

they are elastic up to a point and then slip along slip planes. They also pointed out that the slip planes lie at all angles with the line of action of the load, so that the resolved shearing stresses along the planes will have all values from a maximum in planes at 45 degrees, to zero in planes perpendicular to the load. Thus the crystals will not all slip at once, but one after another. Thus we see that the known properties of the crystals exactly correspond with the assumed properties of the model. Finally, Ewing and Humfrey† showed that when slipping backwards and forwards takes place the slipping surfaces wear and a crack is ultimately formed, which grows till final fatigue failure occurs; thus our criterion for the fatigue limit is correct, viz., that we must have no slipping even in the weakest unit.

But if the model represents the metal so accurately, why does it cease to do so at the yield point? The ultimate strength of a metal is much higher, but the ultimate strength of the model is the same as the yield point. It would not be difficult to alter the model to make it imitate the real metal, but the additions necessary for this purpose would not correspond with any reality in the metal. The rise of strength in the metal above the yield point is, I believe, due to the mutual interferences between the crystals. These must have a great effect

* Phil. Trans. Roy. Soc., Vol. 193A. † Phil. Trans. Roy. Soc., Vol. 200, p. 248.

as soon as there is much distortion, that is after the yield point. The model and my theory are therefore limited to stresses not exceeding the yield point.

But fatigue is not quite so simple as this. Think of a crystal shearing and then of the two parts slipping backwards and forwards on each other; we should expect to find that it required a larger force to shear the crystal than to keep it slipping when once it had been sheared. In other words, that the

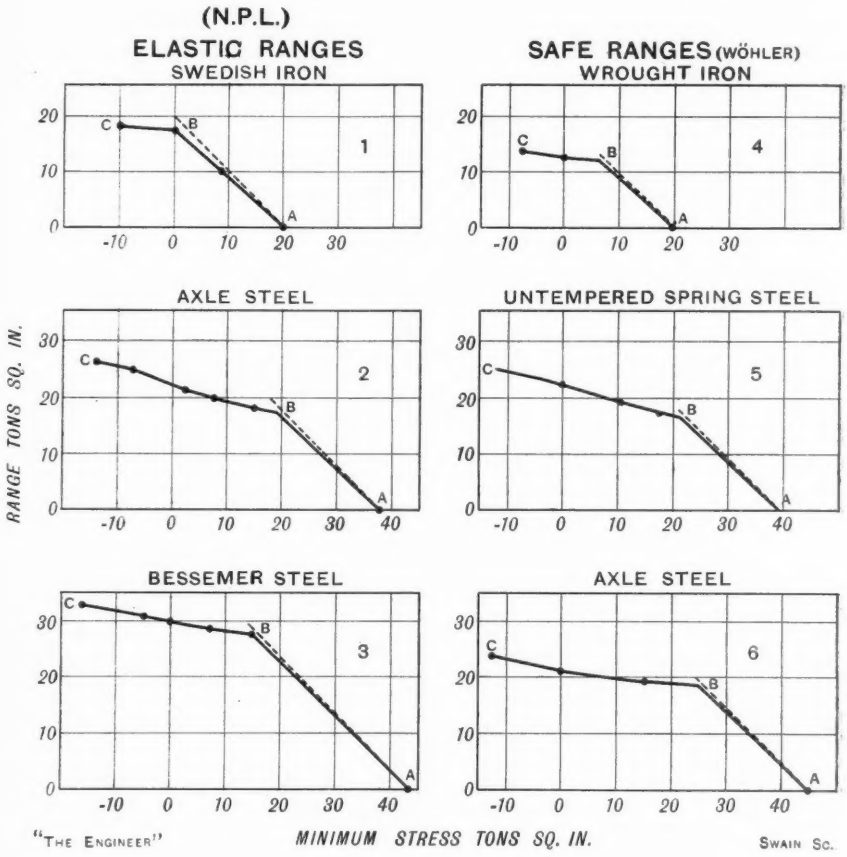


FIG. 9.

adhesion between the surfaces would be greater than the *friction*. Experiments show that this is the case. The model can be altered so as to show this difference by making the sliding blocks grip a collar on the rod, off which they slip when the force is greater than the adhesion. How will this modification of the model affect the results we have already got? You will remember that this modification was assumed in the ten unit model. I have not time to prove it to-night, but you will find that nothing we have so far found is essentially altered till we come to the graph of the complete series of fatigue ranges. This may be altered, but is not necessarily altered. Fig. 10 shows the range graph

for a five-unit model arranged to represent axle steel allowing for the effect of adhesion. Instead of being horizontal at the top, it rises by steps, but these steps may be smoothed out by using more units in the model. Compare this with Bairstow's graph for axle steel, Fig. 9; you will see how closely they agree.

This graph suggested two remarkable experiments. If all the adhesions could be broken down, then the fatigue limit should fall to the value 19.5. This can be done by bringing the metal into the "cyclic condition" by overloading it under alternating stress and then gradually reducing the load. All the adhesions are broken down by this treatment, and the crystals are left in the stress-free condition, as we have seen. Tested in this condition, axle steel should have a fatigue limit 25 per cent. lower than in the normal condition. I tried this experiment on some 0.33 carbon steel. The fatigue limit was lowered 29 per cent.

The second experiment was even more remarkable. If the steel could be made stress-free and then have its adhesion restored, the fatigue limit should be raised to the full adhesion value—some 20 or 30 per cent. above the ordinary value. I took the sample which I have just described, which was in the stress-free condition, and boiled it in water for two hours. This caused the crystals to heal, and so restored their adhesion. I then tested it, and the fatigue limit

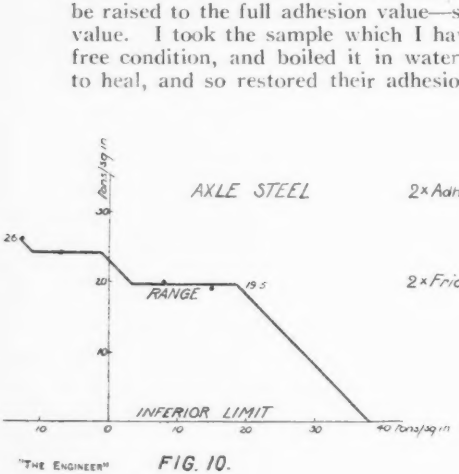


FIG. 10.

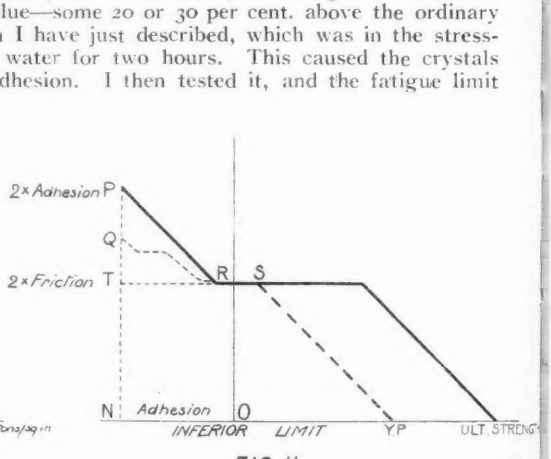


FIG. 11.

was 21 per cent. above the original value. These remarkable experiments show that the fatigue limit of steel can have any value between two limits; they are shown in Fig. 11. Finally, these experiments give a direct measure of the ratio adhesion/friction, which for the steel tested was 1.7.

Other methods may be employed for raising the fatigue limit. If the metal is very slightly overloaded in the Wöhler test for a few seconds and the load then reduced again, the adhesion of some of the crystals is broken down, but so little damage is done that the sheared faces heal at once and the process can be repeated again and again, each time raising the maximum load a little. Gough, at the National Physical Laboratory, raised the fatigue limit in this way 20 per cent. for the steel already referred to, and Professor Lea in Birmingham raised another specimen by rather more.

These experiments show that healing of sheared crystals occurs and that this action may be hastened by very moderate temperatures. Also that the time required for healing depends on the extent to which the slipping faces have been damaged by rubbing. We are thus introduced to a new idea, healing may be taking place during a fatigue test and the ultimate result—fracture or safety—may depend on a sort of race between slipping and healing. Healing may be

thought of as the recrystallisation of the minute quantity of metal disarranged by slipping, and it is not difficult to see why this takes place easily and quickly when we remember that the surfaces between which it occurs are parallel arrangements of crystallised material.

If healing produces an appreciable effect it will raise the fatigue range above the elastic range, and the amount it raises the fatigue range will be greater the more time there is for the action to take place, *i.e.*, the slower the alternations of stress. A second modification of the fatigue range may occur, due to the fact, first demonstrated by Hopkinson, that it takes time for the crystals to slip. Hopkinson showed that for a very short duration of stress, say, one-thousandth of a second, metals were elastic far above their ordinary limit. This phenomenon should also raise the fatigue range above the elastic range, but will be most effective at high speeds. These two actions make it necessary to qualify Bauschinger's general statement on the equality of the elastic range and the fatigue range. The magnitude of the two effects is being investigated; it seems probable that healing may be very effective at the temperatures at which some parts of engines—for example, valve springs—work.

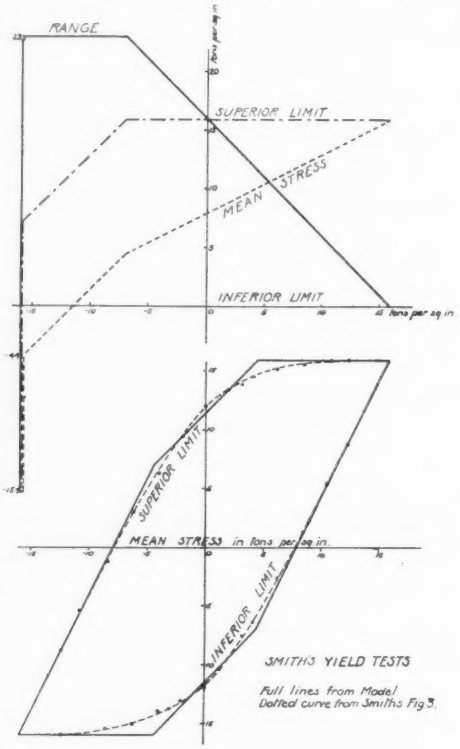
The events occurring in the metal undergoing a fatigue test may be examined in other ways. If there is friction between the parts of crystals there must be heat generated, and we can measure this heat by observing the rise of temperature of the metal. This was first done by Hopkinson, and many observers have used the method since. The temperature rise follows closely on the departure from elasticity, *i.e.*, it becomes large when the crystals start slipping. But there is found to be a very small evolution of heat at stresses far below the fatigue limit, and that the heat gradually increases as the load is increased. In other words, there is a small elastic hysteresis. The model does not indicate this elastic hysteresis, and what it is due to is not yet known; several explanations have been suggested.

Temperature observations have brought to light some very striking phenomena. When nickel is tested in the Wöhler machine there occur sudden heat bursts which rapidly disappear again. They recur every time the load is raised. How can these temporary evolutions of heat be explained? Consider the model. If it were stretched and compressed 2,000 times a minute without lubrication, what would happen? The sliding block would "run hot and seize." While it slipped heat would be generated, but the moment it seized the generation of heat would cease. That this may be a true explanation of what happens in nickel is confirmed by two striking facts. If the heat is due to slipping, heat bursts cannot begin below the elastic limit, for this is the point at which the first crystal slips. Experiment shows that they do not. Again, if a sample is tested up to fairly high stresses and all the crystals which slipped have seized, then if the test is repeated on the same sample no heat bursts can occur. This remarkable result is also confirmed by experiment. The trouble is like an infantile illness—a rapid rise of temperature which a day or two later subsides and immunity from the illness follows for the rest of one's life. Similar heat bursts occur in hard steel, but they last very much longer and are less pronounced.

In 1915 Professor F. H. Smith and Mr. Wedgwood published a paper* on what they call Yield Ranges, illustrated with a very large number of figures showing the results of their tests. I have gone through those figures and find that a model consisting of only three units will reproduce them all. Of course, the figures given by the model are very angular since it has so few units, but that defect could be remedied to any extent by multiplying the number of units. Four of Smith's figures are shown on the screen with the model graphs beside them.

* Journal of the Iron and Steel Institute, Vol. xci.

I am not sure if I have made it clear that the model will give quantitative results as well as qualitative. It is quite easy to construct a model to represent any given metal to scale. To show how accurate the results are, I will give one more illustration. Smith and Wedgwood summarised a large number of their results in a single figure giving yield ranges plotted against mean stress. I designed a model of three units to represent the same steel and constructed to scale the corresponding graph. Fig. 12 shows the graph of the yield ranges, which is very similar to that for the fatigue ranges. On it is plotted the superior stress and the mean stress. Below it, the superior and inferior stresses are plotted



"THE ENGINEER" FIG 12. SWAIN Sc.

on a mean stress base; the figure is a skew polygon. On this, to the same scale, I have drawn Smith's figure representing his test results. The agreement is, I think, striking. If the model could speak it would say: "I could have foretold the whole of the results of the tests which have taken you years to make."

The theory I have outlined applies to iron, steel, nickel and copper. Whether it is true for all metals is not yet known. It is not unlikely that brittle materials like cast iron may behave differently. It ought to apply to fatigue in shear as well as in tension, but whether it does has not yet been ascertained.

Perhaps you wish to know the practical end of all these researches and may feel inclined to say: "What is the use of your pretty models and fine theories?"

We hope before long to be able to put figures into designers' hands which will be a sure basis for all their strength calculations, figures which will not need factors of safety to allow for our ignorance of the strength of the materials. We hope to introduce fatigue limits into steel specifications, so that you will buy your materials on the basis of their useful strengths.

We hope to have methods of fatigue testing as simple as tensile tests.

We hope to be able to issue instructions which will save endless failures of engines.

All these things are already possible; we have now only to verify and confirm before issuing our reports.

I will venture to make one suggestion to-day to all engine builders. Never run a new engine on a full power test till you have raised the fatigue limits of all the highly-stressed parts. This can be done, as Gough and Lea have shown for test pieces, by making a series of short runs at gradually increasing loads, finishing up at the highest overload which the engine will ever be called on to exert and slowing down between each. No marine engineer ever starts up a ship's engine at full load; he coaxes it up gradually. This may be partly to give the bearings time to run in; but the experiments I have described suggest that it may also be most valuable in giving time for the steel to "slip into the central position," and to heal. A proper start may add 20 per cent. to the engine's strength. We shall soon know the best way to make this gradual start. This suggestion is partly guesswork still, but it is the sort of practical result we hope to achieve very soon.

In conclusion, I wish to express my very great obligations to the Aeronautical Research Committee. The Fatigue Panel of that Committee, of which I have the honour to be a member, has been working on this subject since its formation, and all the members of it have been indefatigable in their endeavour to throw light on this most puzzling subject. Researches have been going on at the National Physical Laboratory, Royal Aircraft Establishment, Royal Naval College, and the Universities of Oxford, Birmingham, Liverpool, Manchester and Edinburgh, financed by the Board of Scientific and Industrial Research on the recommendation of the Aeronautical Research Committee. This work is proceeding, and is now advancing rapidly. I have been keenly interested in it since I had to deal with fatigue failures during the war, and I feel sure that the Aeronautical Research Committee are performing a most valuable service to aeronautics by enabling these researches to be carried out.

Discussion

Sir Robert Hadfield said he had come to the meeting intending only to inspect the model which Prof. Jenkin had made, but he had been asked by Prof. Jenkin to hear the opening remarks, the result being that he not only remained, but was deeply interested in the admirable paper on this new method of explaining the behaviour of metals under stress. They were especially glad to have a communication from Prof. Jenkin, because he was one of those who had done so much during the Great War. He (Sir Robert) could not tell them how much Prof. Jenkin had done during the war to help on the technical man. He had had the pleasure of coming into contact with him on a number of occasions, and he (Prof. Jenkin) had helped to elucidate problems, the solving of which had proved of the highest value to the country. Continuing, Sir Robert said that during the past thirty years he had had to deal with the curious metal called manganese steel. This had a very curious property, namely, that, unlike ordinary steel, when a load was put on it, even quite a small load, it immediately began to change its form. If this material showed such an exceedingly low elastic

limit, how was it they could use it structurally? Yet it was so used. There was a certain point where mild steel was better than manganese steel, and yet for certain purposes manganese steel was immeasurably better than mild steel. In other words, the testing machine in such cases did not seem to help us to understand the problem. The other day he had had a communication from M. Frémont, the French engineer, who had done so much with regard to the subject of shock testing, and he (Sir Robert) was rather surprised to see such a question coming from so eminent an engineer. Monsieur Frémont had written and asked what was the reason for the drop in stress at the elastic limit. It seemed to him (Sir Robert) that the points put forward by Prof. Jenkin clearly explained the reason why this drop occurred. He would therefore be pleased to send a copy of Prof. Jenkin's paper to M. Frémont if he might be permitted to do so, because he thought he would find that the explanation now offered would answer the question put.

Prof. Jenkin had dealt with the tensile strengths of adhesion and friction. That was a most valuable fact, and he did not think anyone had ever put that forward so clearly as Prof. Jenkin had done that evening. They were all much indebted to the lecturer for having dealt with the matter so clearly and for having explained his odd-looking but most interesting models. They hoped that Prof. Jenkin would be led to continue his valuable research and give still more information. He (Sir Robert) noticed that Professor Jenkin would like to raise the point of introducing fatigue limits into steel specifications. Those who had to meet the stringent specifications that existed to-day hoped that Prof. Jenkin would thoroughly elucidate all these points, some of which he had admitted were not quite solved, before they were introduced into specifications. He made the suggestion specially because metallurgists were dealing with so many kinds of metal and varieties of steel. He was not speaking of non-ferrous compounds, because he did not pretend to know much about them; but, having in view the enormous range of tenacities of various steels, he did not see how fatigue limits could be introduced into the specifications, except, perhaps, for ordinary types of steel, which were used on a large scale, and which possessed much the same qualities whether made here, in America or on the Continent. He was sure that Prof. Jenkin would be careful before he introduced the absolute definition of fatigue limit, because it was not desirable to add to the difficulties of the manufacturer. In conclusion, he would say that the facts mentioned in Prof. Jenkin's paper would, he felt sure, enable both the steel maker and the engineer to get better and still better results, and he desired therefore to express his indebtedness to Prof. Jenkin for his valuable paper.

Mr. C. E. STROMEYER said the model was a very interesting one. He had seen it at Oxford, and it certainly explained a number of questions in regard to fatigue and elasticity, and allowed one to form a picture of the process going on; but he hoped the model would not become so perfect as to explain everything, because he was afraid that if that were to happen we might get settled down to some definite theories, which would hamper experimental progress. Bauschinger had been referred to, and he believed that Bauschinger's activity had been rather detrimental with regard to this whole question. He had laid down laws and theories about fatigue even before it was known whether a fatigue limit really existed. All that Wöhler had done was to show that a comparatively small stress would ultimately break down a material, but he could not say whether there was or was not a fatigue limit. The first time that the fatigue limit was definitely determined was when he (Mr. Stromeyer) had made his experiments on 27 pieces of steel. He, at the time, tested half-a-dozen examples of each, these being cut from 12in. lengths so as to ensure equality of material. The numbers of revolutions had ranged from about 2,000 up to a million, and, with the help of this steel tested both for bending and for torsion, he was able to construct that law of fatigue which he believed was now well established. He

had not at first got to the fatigue limit itself; but, having completed those experiments, and having found a rough law, he had devised a method for determining the fatigue limit by the detection of evolution of heat. He had in this connection to correct Prof. Jenkin when he had said that he (Mr. Stromeyer) had used the heat method for determining whether the slipping actually took place. What happened was that it had occurred to him that, as heat was generated during fatigue, this would be due to what might be called molecular friction, and if that heating did not take place up to a certain limit, but took place above it, he assumed the boundary would be the fatigue limit, in which case it ought to be in line with the other fatigue results. He found that that was so. Then Mr. Gough had carried out experiments, comparing the temperature rise with the deflection of the samples under the fatigue stresses, and had found that there was a kink as regards this deflection. Therefore, we had three methods of determining fatigue limits. We could find them by a series of actual fractures, we could find them by measuring the heat evolved, and by the extra deflection of the samples. At any rate, it was not until 1914 that we could talk of a fatigue limit. We did not know before then that it existed. All the theories evolved beforehand might be interesting, and it was very nice for people like Bauschinger to discuss possibilities, but he (Mr. Stromeyer) did not think his theories should weigh if they clashed with the subsequent experiments.

With regard to combined static and fatigue stresses, he would like to mention that he had lent a fatigue testing machine to Prof. Jenkin which was the only machine that had yet determined the fatigue limit for these compound stresses. The limit line was a curve, and if the model showed that the line must be straight, then the model showed what he hoped it would show, that it was not infallible and would therefore not discourage experimenters. He had read a paper in South Wales in which he had urged people to make fatigue experiments, and in which he had analysed Wöhler's only reliable experiment on compound stresses, with a view to determining the fatigue limits by extrapolation. The analysis confirmed his (Mr. Stromeyer's) experiments about the fatigue limit lines being curved and not straight. It was interesting to note that one sample stood 800 repetitions under a compound stress which was 1½ tons above the ultimate breaking strength of the material. It was an interesting case, because it showed that there was some sort of freezing going on even during the fatigue, and it knocked some of Bauschinger's theories on the head altogether. He again hoped that the model would not be too perfect. He hoped that Mr. Gough would say something about the raising of the fatigue limit. He himself had not explored that field. He had made a very large number of fatigue tests and had measured the heat evolved during the life of the samples from the very beginning until the sample had broken, but had never noticed that the fatigue limit was raised. When once he had got past it, heat was generated, and the generation went on until the sample broke. The elastic limit, he believed, might be raised, and he believed some experimenters in America had proved this.

Prof. JENKIN said that they had actually raised the fatigue limit.

Mr. STROMEYER said he expected they would find that something must happen when they dealt with a combination of a very high permanent load and weak fatigue stresses. Evidently if the static elastic limit was passed, either the fatigue limit had also been passed and could not be recovered or a new fatigue limit would be created. Possibly this would require time—a sort of ageing process would be progressing. He had not been able to carry out that experiment, but he hoped Prof. Jenkin would do so.

Mr. H. GOUGH said that in going through all the published work on this subject he had seemed to get into a morass, until he had come to that paper which was the finest paper on fatigue ever written—that of Prof. Bairstow in

1910. At his department there were three men who were very keen on the subject of fatigue, and they had almost formed a society for discussing Bairstow, because every time they went through the paper they found something new and important. But there were two apparent inconsistencies in the theory of the subject that were, he believed, now cleared up. They were the question of perfect elasticity and the association of the appearance of slip lines at the limiting range of stress. Dealing with elasticity, the experiments of Guest and Lee, Hopkinson, Rowett and other workers showed that perfect elasticity was never obtained. He believed Prof. Bairstow would agree that the elasticity referred to in his paper was elasticity as consistent with elastic hysteresis. The other point was that researches had been made by Ewing, Rosenhain and others, who, although very cautious in their deductions, led one to believe that the first sign of the limiting range of stress having been exceeded was the formation of slip bands. A colleague of his, Mr. Hankins, had some time ago tested nickel and had obtained the extraordinary result that the fatigue range, under reversed direct stresses, was more than twice the range of the yield stresses in tension and compression. Thus a piece of nickel under alternating stress had been taken past the range of the elastic limits and of the yield points, and, presumably, no slip bands had been formed in the material. This they could not understand. Again, lately in America some researches on fatigue had been completed, one material used being "Armco" iron. It was practically a pure iron, with an ultimate tensile strength of about 19 tons per square inch, a yield stress of about $8\frac{1}{2}$ tons per square inch, and a fatigue range under reversed bending stresses of ± 11.7 tons per square inch. He was rather interested in their results and had obtained some Armco. In collaboration with a colleague, Dr. Hanson, he had been examining it under a microscope and was pretty well convinced that as higher magnifications were used, so more and more indications of slip would be seen on the surfaces examined, in the same way that more refined methods of measuring strain showed greater deviations from perfect elasticity. They had taken some specimens of Armco (possessing a limit of proportionality and yield stresses at 10 and 11 tons per square inch respectively), tested them under simple bending stresses in a Wöhler machine, and examined prepared surfaces at all stresses up to and exceeding the fatigue limit. The results were not yet reported on, but the tests showed most conclusively that once the elastic limit was passed, *i.e.*, a range of applied stress that was double the static limit of proportionality, undoubtedly curved lines appeared on the polished surface. Dr. Rosenhain had pronounced these curved lines to be true slip bands. Once the yield range was exceeded, there were undoubtedly thousands of slip bands, and they were fairly certain that slip bands appeared under a stress that was even less than the static limit of proportionality. Prof. Bairstow had used, in addition to reversed stresses, ranges of stresses in which the upper limit of stress exceeded the static yield point, and if he had used a microscope he would have found slip bands formed on the surface of the material. It boiled down to this—that with Prof. Jenkin's model, all the apparent inconsistencies were absolutely cleared up, and he wished to add his congratulations to Prof. Jenkin on his excellent paper.

Prof. C. H. Desch said that the conclusions expressed in Prof. Jenkin's Paper were of the greatest interest to the metallurgist. The behaviour of the model corresponded in the most wonderful manner with what was known of crystal structure at the present time, and he had been unable to detect any flaw in its application to metallographic theory. In the older treatment of the strength of materials, metals had usually been regarded as isotropic substances, but as a matter of fact their properties were different in different directions. It must be the case that when stress was applied it would not be equally distributed over all the crystal grains, but that some would be less favourably placed than others. In some work which was being done at Sheffield they had tried to follow the behaviour of a piece of metal while under tensile stress by using a polished sur-

face and keeping it under microscopical observation during stress with the object of detecting the formation of the first slip bands. This proved to be difficult to do, and a new method was devised by one of his research students, Mr. Cecil Handford. This method consisted in using a flat polished test piece, on the surface of which was laid a piece of mica, and on that again a strip of brass, the whole being clamped together and constituting a small electrical condenser. This was connected with a thermionic valve, oscillating with a certain frequency. At a short distance was placed a second oscillating valve, giving with the first beats of, say, 100 per second. The note thus produced was intensified by a two-valve amplifier, and was then heard in a telephone. The idea was that as soon as the first slip occurred there would be a minute ruffling of the surface, which would alter the capacity of the condenser. It was found that this actually occurred, and that the first production of slip bands could be recognised by a sharp rise in the pitch of the note in the telephone. He believed that in Mr. Stromeyer's method of testing, nickel gave out heat at a certain stress, then became elastic again, and then yielded again at a higher stress. This behaviour of nickel had also been observed by Mr. Handford's method.

In his account of copper, Prof. Jenkin had assumed that there were decided stresses in the individual grains of the annealed metal, some being in a state of internal tension and others in compression. He was inclined to doubt whether there were really such stresses, or whether neighbouring grains did not differ greatly in the ease with which they yielded under the stress, on account of the varying orientation of their crystal planes. If that were the case, one would expect to find considerable differences between the behaviour of specimens differing in the size of their grains. That explanation would not apply to the hardened steels, the grains of which were exceedingly small. With regard to the behaviour of metals beyond the yield point, he did not think that metallurgists would be greatly troubled with difficulties in that region, as the whole course of events could be understood on existing theories now that the information given by the X-ray method was available. It was the behaviour below the yield point, especially between the fatigue limit and the yield point, that presented so many difficulties, and that had now been so remarkably elucidated by the work of Prof. Jenkin.

The CHAIRMAN said that, so far as his experience of fatigue went, the model which Prof. Jenkin had shown accounted for practically everything. One of the difficulties he had always felt in connection with fatigue—and it was a matter which he was not able to take up because of the advent of aeronautics—was the possibility of reducing the yield point. It was quite clear, from his own experiments, that if the range of stress exceeded the fatigue limit, then premature yield could be produced; but he had not been able to produce yield by the application of a range of stress less than the fatigue range. One other point had always puzzled him, and Prof. Jenkin had not dealt with it that evening, and that was how did a specimen fail when both the limits were compression? The specimen became thicker and thicker, and more able to stand the load, and he could not understand how fatigue could produce failure. Perhaps Prof. Jenkin would be able to give some information on the point. Generally speaking, he thought they would all agree that the subject had been made a great deal clearer by the fact that Prof. Jenkin had found that observed phenomena depended on two important factors—a coefficient of adhesion and a coefficient of internal crystalline friction—and that these factors, utilised in a simple model, explained much that had previously been wholly empirical.

Prof. JENKIN, in reply to the discussion, dealt first with Sir Robert Hadfield's question as to manganese steel and its low elastic limit. He believed he had shown by the model that the elastic limit was practically an accidental point and had no importance whatever from any user's point of view. The points that were important were the fatigue limit and some of the other points, but not the initial elastic limit. That was more or less accidental, so that he did not think

Sir Robert need feel any anxiety about the merits of manganese steel because its elastic limit was low. He believed that Sir Robert Hadfield had had enough experience of his (Prof. Jenkin's) methods of dealing with specifications during the war to relieve him of any anxiety about his issuing them too soon, but he did intend to get fatigue limit into specifications. (Laughter.)

Mr. Stromeyer's remarks were interesting, but he believed that he disagreed with almost all of them. It was unfortunate when one's theory said that one could not do a thing which had been done experimentally. (Laughter.) He had done things which Mr. Stromeyer considered were impossible, and he believed he could claim to be one of the first to have done them. The raising of the fatigue limit was a point beyond discussion. It had been done by Prof. Lea, who had applied a very large number of repetitions, 40 million reversals of stress, 20 per cent. above the initial fatigue limit. Similar results had been obtained with even higher stresses by Mr. Gough at the National Physical Laboratory. He was particularly pleased to hear from Mr. Gough that the model had cleared up some of his difficulties. It was very difficult in discussing this subject to follow the arguments, and he did not quite follow what Mr. Gough meant. He was particularly pleased also to find that Prof. Desch considered that the model fitted in with metallurgical requirements. He had been urging metallurgists to deal with that side of the problem, but so far he had had very little response. The crucial point in the theory was the initial stresses in the crystals, as Prof. Desch had quite rightly suggested. What these were due to it was for him (Prof. Desch) to say, and not himself. He had suggested that a possible cause of the initial stresses might be the unequal contractions of the crystals along different axes. He was not metallurgist enough to say whether that was sufficient or not. Dr. Rosenhain had suggested a possible modification of the idea of initial stresses, namely, that some of the crystals might take the load before the others. He believed he (Prof. Jenkin) had devised a test which would determine whether that was true or not. He would be surprised if it turned out to be a possible explanation. It seemed impossible to him to conceive any doubt about the initial loads on the crystals when they saw them slipping under very low loads. The crystal must be under load if, the moment they put on more, it began to slip. That was a very convincing experiment, to his mind. He had not thought of the point raised by Prof. Bairstow about the fluctuating compression. That point would have to be considered; but the model had already suggested such a number of lines of research that were necessary that it was difficult to make a promise as to how soon he could take up that one. He thanked the members for the very kind way in which the paper had been received.

The meeting then closed.

JUVENILE LECTURE—MODEL AIRCRAFT

The following lecture was delivered by R. A. FRASER *before the Society on January 11th,* 1923.

Types of Aircraft

I have not prepared a formal lecture for this afternoon. Instead, I hope to interest you in a few experiments with models of aircraft.

In the first place, however, it may be useful to look at one or two pictures of actual aircraft, and at the same time to compare these parent machines with their smaller brethren, the models.

Let us commence with the largest known type of aircraft—the rigid airship; the R.38, for instance, was nearly 700ft. in length. With airships of this type, shape is preserved by means of an elaborate framework of light metal girders, and the whole weight of the machine, including the powerful engines, adds up to many tons. Whilst the airship is flying level, this enormous load must, of course, be kept lifted and balanced by some upward force. The upward force is called "buoyancy," and is similar to that which keeps a ship at sea from sinking; it is provided, in this case, by the volume of hydrogen contained in the gas bags of the airship.

Let us now imagine that we have successfully balanced an airship, such as the R.38, so that it is floating steady and level in still air. In order to drive the airship forwards against the resisting force of the air, we should have to exert a considerable forward push. For instance, if we wished to keep moving at a speed of 50 miles per hour, we should have to drive with a force of about two tons. During steady flight, the engines of the airship are allowed to turn half, or all, of the airscrews, and these provide the forward thrust constantly needed to balance the air resistance. On a model airship 1/200th the size of the R.38, the air resistance at 50 miles per hour would amount only to some two ounces.

Other smaller types of airship, such as the S.S.Z., exist in which form is maintained by gas pressure and not by a rigid girder-work. In a wind of 50 miles per hour the air resistance on a fully rigged model, 1/50th the size of an actual S.S.Z., would be about six ounces. Roughly two-thirds of this whole resistance acts upon the cars, fins and rigging, and only the remaining one-third upon the hull.

Let us now turn for a moment to the large commercial aeroplane, such as a typical Handley Page machine, in which the wing span extends to 100ft. During horizontal flight at 100 miles per hour the air resistance would be as much as two tons, and the air lift on the wings over 11 tons. This air lift is the force which balances the weight of the machine—fulfilling much the same purpose as the gas lift on the airship.

As a final example we might refer to the parachute. This is, perhaps, peculiar amongst aircraft in that its shape is designed to yield a large, rather than a small, air resistance. Once filled with air and dropping fully open, the parachute experiences an upward resisting force, which so usefully helps to balance out the weight of the passenger.

How Wind Forces are Measured

I now want to give you a rough idea as to *how* the wind forces which act upon these aircraft models, can be measured. Later on I shall attempt to show you *why* a knowledge of the forces is really helpful to the designer.

Before any exact measurements can be attempted, a thoroughly steady air current must be provided. A "wind tunnel," or "channel" is used for the purpose. You can regard a wind tunnel quite simply as a long tube, through which a stream of air is sucked by means of a propeller driven by an electric motor. Careful precautions are, of course, taken to ensure a thoroughly steady and reliable flow. The air which circulates from the outside is often much disturbed and apt to contain numerous eddies or air swirls. Such irregularities at the intake mouth can be straightened by means of a honeycomb grating placed at the mouth of the tunnel, and an even flow is thus secured. It is also found most effective to allow the air which has already passed through the tunnel to escape or diffuse back into the containing room through a group of parallel slots, spaced in a particular manner. For test the models are supported by means of spindles or wires in the most uniform portion of the air current, the forces exerted by the wind being measured out on accurate balances arranged sometimes above, sometimes below, the wind tunnel.

At the National Physical Laboratory the largest wind tunnel measures 7ft. by 14ft. in section; its containing room is 150ft. in length. Perhaps you do not fully realise the enormous air movements which take place when such a tunnel is running at high speed. At 70 miles per hour, for instance, over 20 tons of air pass through the channel in every minute.

Before the tunnel can be made of real use, some method must be found to determine the speed of the wind. I shall not attempt to explain in exact detail how this is accomplished. Generally, use is made of air pressure such as that which acts at the mouth of an open-ended pipe held facing the wind. If the other end of such a pipe be connected by means of a length of rubber tubing to a glass U-tube containing some convenient liquid, air pressure will be communicated to the liquid and bring about a difference of level between the two columns of liquid. For each wind speed there will be a new condition of balance, and so we might expect to find some relation giving a measure of the wind speed in terms of the difference of level. This, with certain modifications, is the principle actually adopted.

In order to find the *direction* of the wind, an instrument is used consisting of a pair of such open-ended pipes inclined at an angle, and connected to opposite limits of a U-tube. Suppose this instrument to be turned until no difference of level can be noticed between the liquid columns of the U-tube. The measured air pressures must now clearly be equal, and you will readily guess that the true wind direction must, therefore, equally divide the angle between the open-ended pipes.

Perfectly steady flight is not the only condition which has to be allowed for in tests upon aircraft models. Actual aircraft in flight are always liable to meet with small disturbances, such as atmospheric gusts or cross-currents, and a question of very great practical importance is the future behaviour of a machine after such irregularities have acted. Some aircraft recover their original condition of flight after disturbance; whilst others might, for instance, begin to take up an ever increasing swinging motion, and in time become quite unmanageable. I hope shortly to give you some examples of such different kinds of motion, by dropping a number of small parachutes. This branch of the subject contains, of course, many interesting and difficult problems, which call for very special arrangements and devices in the wind tunnel; but I shall not be able to tell you these methods this afternoon.

Flow-Patterns

You will perhaps be wondering how it is that measurements made upon a small model can teach us anything useful about the forces upon a full-sized machine. A complete explanation would not be altogether simple, but in a rough and very indirect way I hope to convince you that laws exist, under which one

might reasonably expect to fix some connection between the small and the large machines.

The wind forces acting upon any body are a direct outcome of the disturbance caused by the body to the passing stream of air. A careful comparison between such stream disturbances might, therefore, bring to light the required laws. Air moves past obstacles in a way similar to the flow of water, but with both fluids the stream disturbance or so-called " flow-pattern " is invisible. You have all seen, at one time or another, a smoke ring; it is really an air-ring—a very special type of air flow. The air-ring carries smoke along, and the flow becomes visible to your eyes. Perhaps not all of you have seen a water-ring. You can quite simply produce and make visible such a ring by allowing a single drop of coloured liquid to fall from a small height into a basin of water. If the coloured liquid is fairly heavy compared with water, you will find that the ring cannot last in a perfect form for very long. It will soon bud out into a set of smaller rings, and each of these will in turn continue budding (Fig. 1). These are only simple

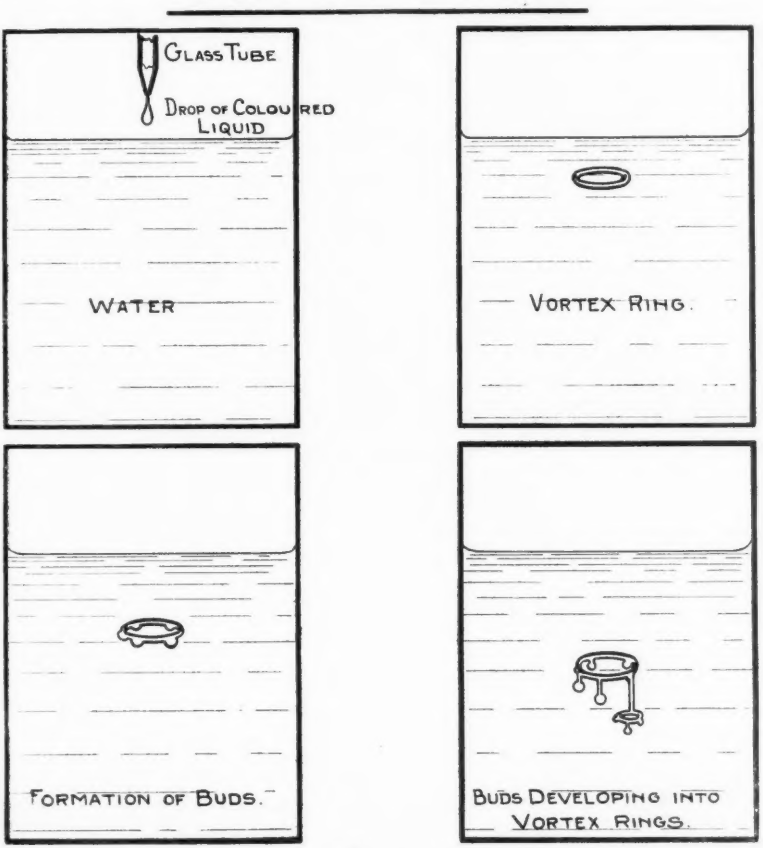


FIG. 1.

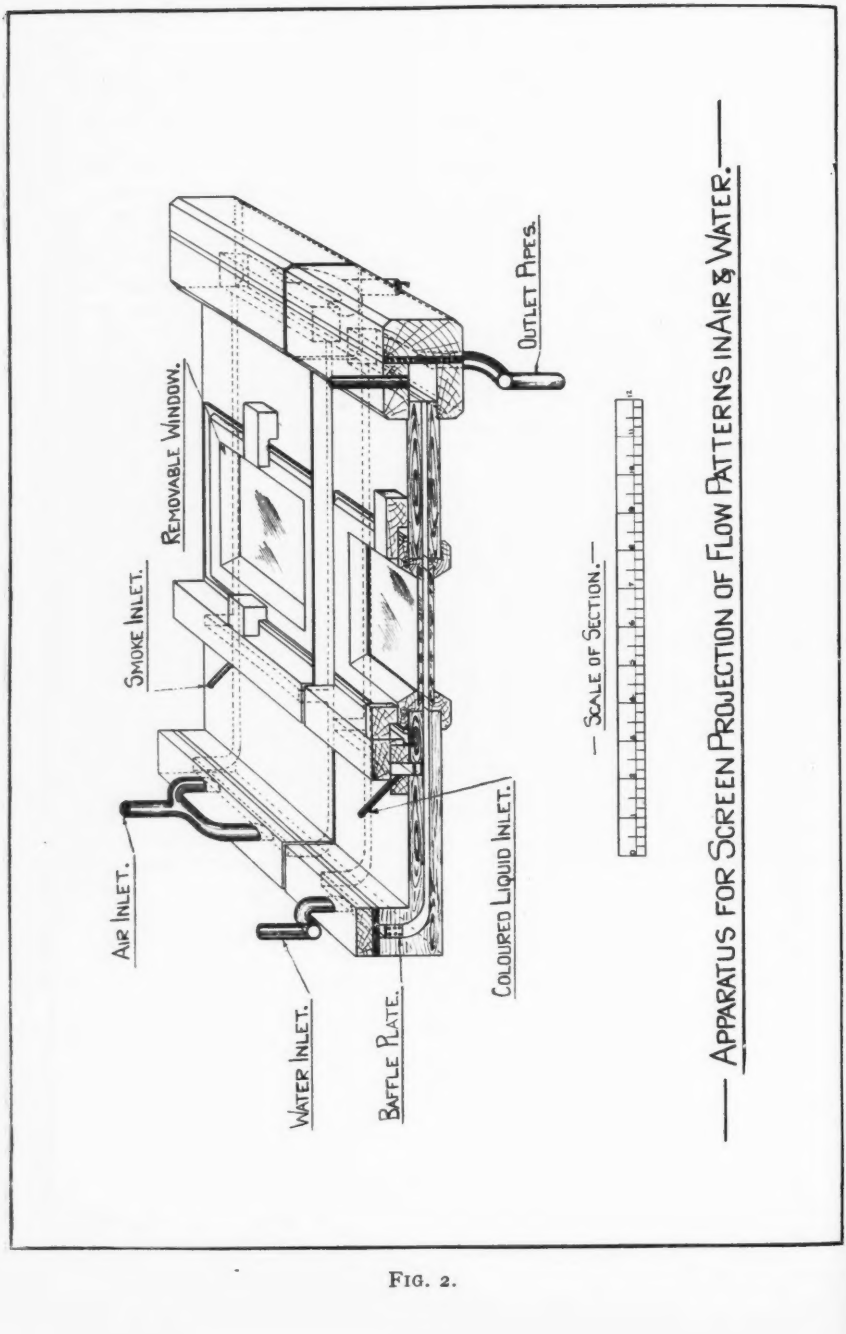

Apparatus for Screen Projection of Flow Patterns in Air & Water.
Air Inlet.
Smoke Inlet.
Removable Window.
Outlet Pipes.
Water Inlet.
Baffle Plate.
Coloured Liquid Inlet.
Scale of Section.

FIG. 2.

examples, but they suggest how flow-patterns can be shown up clearly with use of smoke or coloured liquid.

The apparatus I shall make use of in order to throw flow-patterns upon the screen, can be thought of most simply as a pair of shallow closed glass boxes, forming separate channels, $\frac{1}{8}$in. deep, for a uniform air-current and a water current. (The arrangement is shown in fuller detail in the perspective sketch, Fig. 2.) Smoke can be introduced into the air current through a transverse row of small holes, and coloured liquid (methylene blue), through a similar set of holes, into the water current. The two boxes can be used very much like ordinary lantern slides.

With no model introduced, you would, at a low speed, merely see upon the screen a row of parallel straight streamlines, along each of which smoke, or coloured liquid, was being steadily carried down channel with the stream. If a model, however, be placed in position, the current is deflected by the obstacle, and the streamlines become pushed aside, forming a curved flow-pattern.

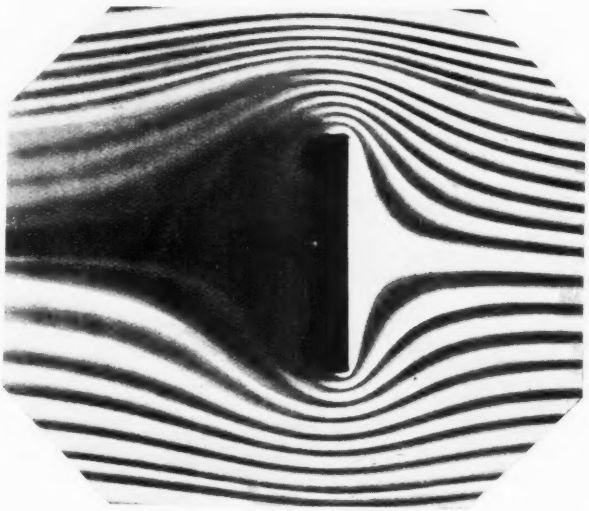


FIG. 3.
Flow pattern with flat plate (water).

Many important points concerning the nature of the flow can be illustrated by a careful study of these pictures. For instance, by regarding each streamline as steadily draining its own particular supply of colour, we should naturally suspect at places where the line widened a lower speed than at the narrow or constricted places. A glance at the flow pattern will, therefore, tell you how the speed changes from place to place in the current. Again, with some pictures, a tail of colour, practically motionless or stagnant, appears to have become trapped at the rear of the model. The extent of the stagnant region, which is called "dead," should tell something about the resistance of the model. With a square plate held normally across the stream (Fig. 3) the "dead" region is large, furnishing a high resistance. You will obtain similar effects, in a smaller degree, with a circular model (Fig. 4), or with an aeroplane wing section held at a large angle to the current (Fig. 5). When the wing section is inclined at only a small angle

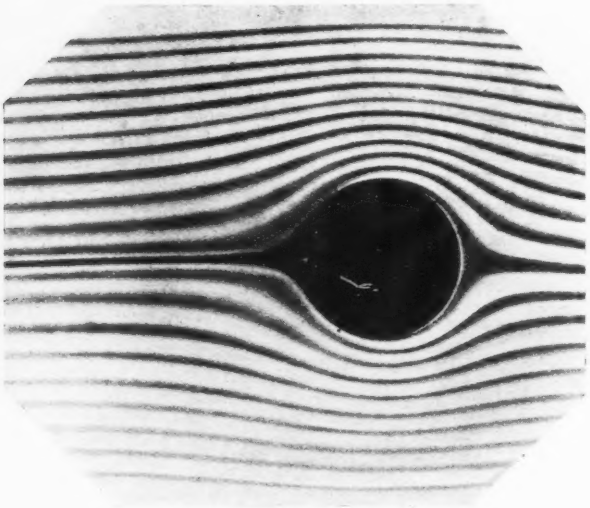


FIG. 4.
Circular cylinder (water).

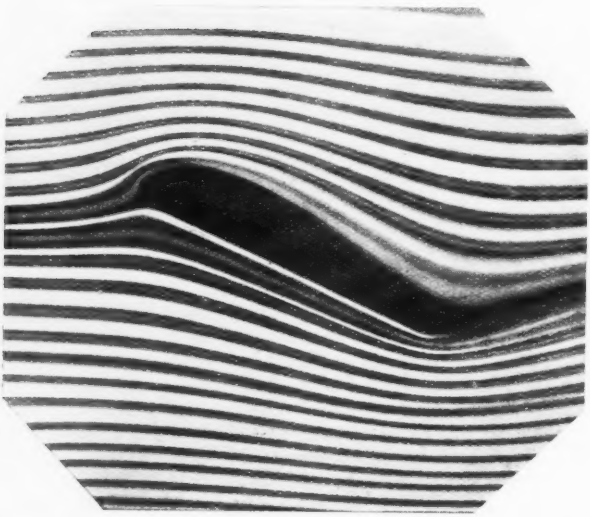


FIG. 5.
Wing section at large incidence (water).

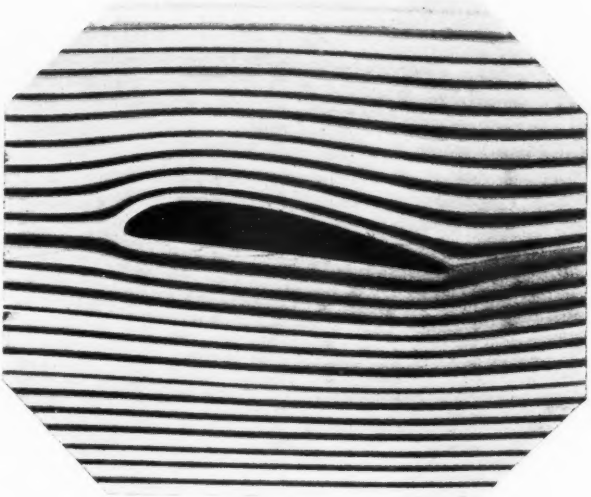


FIG. 6.
Wing section at small incidence (water).

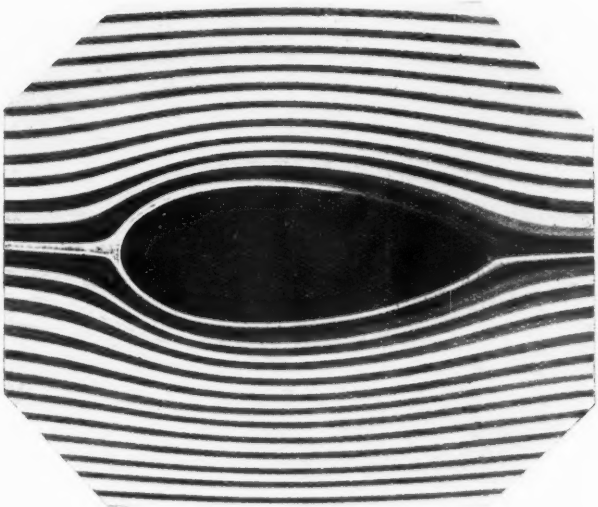


FIG. 7.
Strut section (water).

(Fig. 6) the resistance becomes much reduced, and the "dead" region scarcely noticeable.

Under certain conditions, many curious effects can be obtained. I shall only draw attention to one, which is most effectively shown with the water channel. It can be obtained best by first closing the water supply and allowing the channel to flood dark with colour. The water current is then allowed to run, and gradually wash the curtain of colour away from the model. A marked "halo" of perfectly clear water will soon develop round the nose of the model, showing up distinctly against the still deep colour of the main stream. The study of flow is rich in puzzling problems, and this "halo" is one of many for which you might care to try and think out an explanation.

If we were to make a careful collection of these different flow-patterns, we should find that every pattern possible with a given model in air could be repeated, although not necessarily at the same speed, with the same model placed in water.

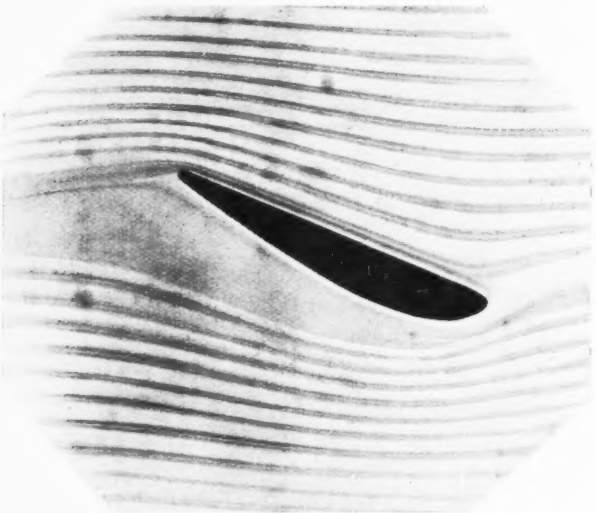


FIG. 8.
Wing section at large incidence (air).

In other words, our picture gallery for water would include every picture we could hope to compose in the air current (compare Figs. 5 and 8). This is only a simple case of a more general law which allows us to substitute in the above way one fluid for any other fluid.

We are, therefore, able to confine our attention to pictures obtained with one special fluid only, such as water. For a given *shape* of model, we could compile sets of pictures at different speeds, each set obtained with a different *size* of model. With a large model we would, of course, obtain large-sized flow-patterns, and with a small model correspondingly smaller patterns. But every pattern possible with the large model could be repeated in shape, although not necessarily at the same speed, with the small model. A sufficiently complete collection of pictures for the small model, would, therefore, enable us to predict results for the large model. Since the forces acting are directly connected with these flow-patterns,

some general law can be suspected connecting measurements on models of different sizes. A law of this nature does actually exist. It lies at the root of all wind-tunnel measurements, and provides for the designer the all-important link between the factors which enable him to design his machine and the measurements which can so conveniently be carried out in the wind-tunnel.

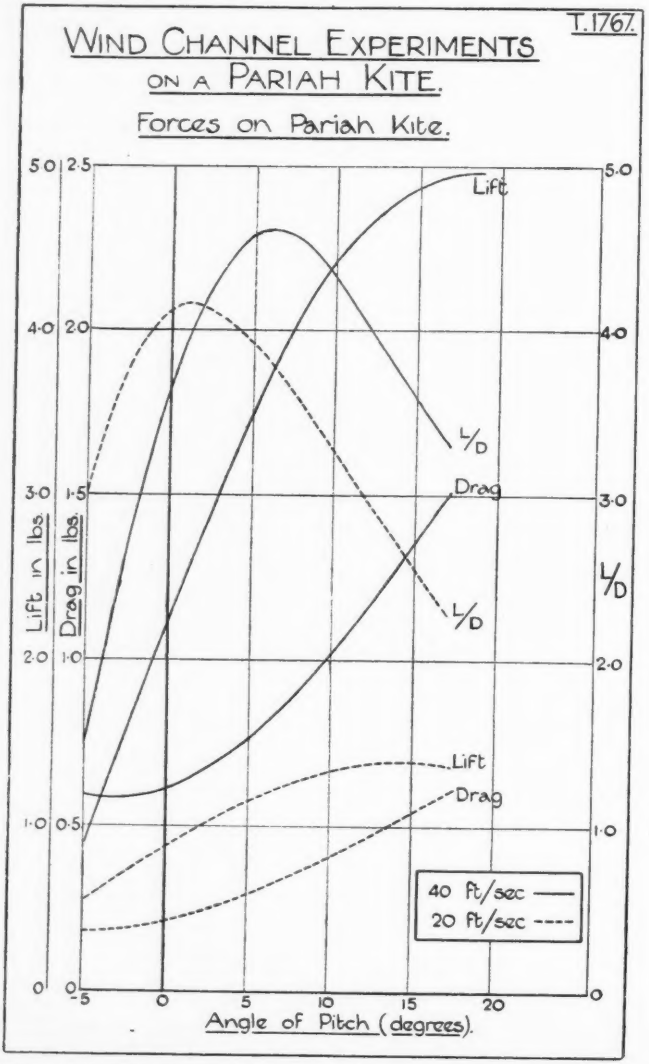
The points touched upon in the above written communication were illustrated by lantern slides and by experiment; a number of models were also exhibited. A 1ft. wind channel was shown in working order, simple force measurements and speed regulation being demonstrated. Model parachutes were dropped in illustration of types of instability. The formation of vortex rings and buds in water was dealt with by projection upon the screen, both plan and elevation views being shown. A number of representative flow-patterns were obtained in air and water, these pictures concluding the lecture.

(The lecturer is indebted to Mr. A. J. Webber, of the National Physical Laboratory, for his assistance in preparing the above lecture.)

WIND CHANNEL EXPERIMENTS ON A PARIAH KITE *

BY W. L. LE PAGE.

T.1541 contained a description of experiments carried out on an Alsatian swift, a bird of small dimensions and of a "flapping" species. It was thought desirable to repeat the experiments with a large bird and one belonging to the soaring species common in tropical countries.



* T.1767. Printed at the request of the Aeronautical Research Committee.

The pariah kite, or cheel, is reported to be a very efficient bird and, at the same time, one of the commonest of the class. The specimen obtained for these experiments had a span of 53¼in., and measured 24in. from the tip of the beak to the extremity of the tail. The chord of the wing varied from 10.75in. at the body to 0in. at the tips. A wooden former was arranged in the body for the purpose of taking a steel sting, which extended to the end of the tail feathers. The wings were externally braced by means of a strip of metal placed round the body and continuing along the undersurface of each wing for a distance of about one-third of the half-span on each side. This bracing device was concealed as much as possible in the feathers, as will be seen from the photagraphs,* which also show the symmetry of the bird to be moderately good.

The customary methods for testing aerofoils were adopted. The bird was supported in a 7ft. channel by two wires from the main lift balance to the metacarpal joints of the wings, the sting being connected in the usual way to the rear vertical force and drag balance. The preliminary experiments were carried out with the bird in characteristic gliding attitude. Lift and drag forces were measured over a range of incidence, at wind speeds of 20ft./sec. and 40ft./sec. The incidence of the wings relative to the body was then increased, and a further test made.

It was thought that the most efficient disposition of the wings might not have been attained in this manner, and a few tests were therefore carried out with the body of the bird at the angle of minimum drag, the incidence of the wings alone being varied. The original results were, however, better, and only these are included in the present report.

Results

It will be seen from the appended table that results comparable with those of the Alsatian swift (T.1541) have been realised. The investigation generally confirms the poor results obtained in previous tests on birds. The maximum lift/drag ratio is 4.6, indicating a gliding angle of 12.2deg., which compares very poorly with that of recent gliders.

During the wind channel work it was noticed that the trailing feathers of the wings were being flexed upwards by the wind pressure; the reversed curvature started at a point approximately one-third of the chord from the leading edge, and was of considerable magnitude. This fact might account for the inferior results obtained, if the bird were known to have the power of keeping its feathers straight and unflexed, since it has been shown that reversed curvature in an aerofoil has a detrimental effect upon the maximum L/D ratio. (R. and M. 72, 1912-13).

TABLE I.

FORCES ON PARIAH KITE.

Angle of pitch (Degrees).	20ft./sec. Lift (lbs.).	20ft./sec. Drag (lbs.).	20ft./sec. L/D.	40ft./sec. Lift (lbs.).	40ft./sec. Drag (lbs.).	40ft./sec. L/D.
—5	0.542	0.180	3.01	0.890	0.595	1.50
—2	0.750	0.194	3.87	1.690	0.589	2.87
0	0.875	0.213	4.11	2.230	0.612	3.64
+2	0.995	0.241	4.13	2.745	0.659	4.17
4	1.102	0.275	4.01	3.245	0.724	4.49
6	1.195	0.314	3.81	3.715	0.807	4.61
8	1.270	0.359	3.54	4.115	0.906	4.54
10	1.327	0.408	3.25	4.415	1.024	4.31
12	1.372	0.460	2.98	4.645	1.149	4.04
14	1.390	0.513	2.72	4.810	1.287	3.74
16	1.385	0.571	2.42	4.915	1.429	3.44
17	1.365	0.599	2.28	4.940	1.501	3.29

* Not reproduced.

AERONAUTICA*

The Royal Aeronautical Society (7, Albemarle Street, Piccadilly) is endeavouring to form, what it ought to have possessed long ago, a comprehensive library of books and prints on every phase of aerial flight. Thanks to a substantial grant from the Carnegie United Kingdom Trust, a very fine selection of the older books has been secured on satisfactory terms from Messrs. Maggs, who have made, as indicated in this column on January 22nd, 1920, a speciality of aeronautical books. The whole of the books and prints described in their catalogue issued three years ago was purchased *en bloc* by the late George D. Smith, of New York, and passed into Mr. Henry E. Huntington's collection. But duplicates of practically all the earlier books were obtained by Messrs. Maggs, and are now with many others on the shelves of the Royal Aeronautical Society, whose Secretary, Lieutenant-Colonel W. Lockwood Marsh, O.B.E., is also a keen private collector of such books. Important collections of books on aeronautics may be found in the British Museum, the Patent Office, and in the Science Library of the Victoria and Albert Museum, but it may be doubted if any of these libraries could produce anything like all the titles registered in Tissandier's " Bibliographie Aéronautique," Paris, 1887; and since that date a vast mass of printed and artistic matter has been issued not only in Europe, but also in America and elsewhere. Aerial navigation has undoubtedly come to stay; and as very few of the earlier books on the subject were printed in large numbers, they have all become rather rare. The ever-increasing number of those who collect such books, public institutions and private individuals, has naturally much enhanced market values. The extraordinary variety and interest of the subject are fully demonstrated in Tissandier's sumptuous " Histoire des Ballons et des Aéronautes célèbres," 1783-1880, and in the Comte de la Vaux's equally splendid continuation volume recently published.

Which was the first published treatise on navigation in the air? The idea itself was much older than Blanchard, Montgolfier, and other modern pioneers, for it occurred to the Persians nearly two thousand years ago. Messrs. Maggs have a page from a Persian MS. in which a king is represented shooting at birds from a floating car. One of the earliest books in the Royal Aeronautical Society's library is the first of several editions of Francesco de Lana, " Prodomo overo saggo di alcune inventioni nuove," etc., a folio printed at Brescia, 1670, with twenty fine plates, including one of the " aerial ship." This is well worthy of being described as the First Folio of aeronautics; a translation of the fifth and sixth chapters was printed by the Society in 1910. A counterblast, perhaps not the first, to Lana appeared at Rovereto in 1753, in Cavalcaho's " L'Impotenza del Demonio di trasportare a talento per l'aria da un luoga all'altro i Corpi Umani," in which the author declares that " the atmosphere has always been unknown to man, and will continue to be a region unknown to him. No one, not even the Demon himself, has the power to teach man any method by which he may explore that region, either by increasing his motive power or by diminishing very considerably his specific gravity." Another early book, also in the library, is Antonio de Fuenta la Peña's " Discurso," " El Ente Dilucidado," Madrid, 1677, a substantial quarto of over 500 pages, and the subject of an article by Señor Vicente Castañeda y Alcover in the " Revista de Archivos, Bibliotecas y Museos,"

* Reprinted from the " Times " literary supplement.

1916, with the title "El Primer libro impreso sobre Aviación ¿es Español?" Another early author is J. A. Borelius, "De motu animalium," Rome, 1680-1, which went through several editions, and is remarkable in having a chapter, "De Volutu," on the theory of the flight of birds and on aviation, in which the author comes to the conclusion, "Est impossible ut homines propriis viribus artificiose volare possint." This section of the book was translated and printed by the Society in 1911, with copies of the curious diagrams—Leonardo da Vinci's earlier work on the subject of the flight of birds was then unknown and was undiscovered until the end of the last century. Earlier than any of these is the Veranzio, "Machinae Novae Fausti Verantii Siceni," a folio published at Venice about 1595, with an engraved title and woodcut border showing the flight of Daedalus and the fall of Icarus, and containing on p. 13 the first description of a parachute.

The idea of aerial navigation was in the minds of men, scientific and otherwise, throughout the seventeenth and eighteenth centuries, long before ballooning became an accomplished and practical fact. The Marquis of Worcester, in his "Century of the Names and Scantlings of such new Inventions," London, 1663, wrote of "how to make a man fly; which I have tried with a little boy of ten years old in a barn, from one end to the other, on a hay mow." Three years later we have a romance from the pen of Francis Godwin, Bishop of Hereford, "L'Homme dans la Lune," Paris, 1666 (a new edition appeared in 1671), of which the hero flies to the moon with the help of an apparatus which is carried through the air by ten "Gansas," wild geese or swans. Robert Paltock's "Life and Adventures of Peter Wilkins," 1751 (translated into French and issued in Paris in 1763) is a well-known air book, and may have suggested Dr. Johnson's chapter "A dissertation on the art of Flying," which appears in "The Prince of Abbissinia (Rasselas)," 1759. In the interval we have, among other things, Samuel Brunt's "Voyage to Cacklogallinia," London, 1727, with a curious plate of the author being carried through the air in a flying machine supported and escorted by cocks. In the region of romance also, there is another interesting early English item, "The Scribleriad: an Heroic Poem in Six Books," London, 1751, with several plates, one depicting an aerial combat between an Englishman and a German, a rarity not known to Tissandier or any other modern bibliographer of aeronautical books.

The most prolific period of printed matter concerning aerial navigation dates from 1783, and in connection with this mention may be made of Joseph Galien's "L'Art de Naviguer dans les Airs," Avignon, 1757, an excessively rare book, in which the author suggests filling large cloth balloons with rarified air of the high regions, gathered on mountain tops—probably the first real instance of the mention of balloons.

REVIEWS

Les Hélicoptères

Par W. Margoulis. Paris: Gauthier-Villars et Cie., Quai des Grands-Augustins, 55. (pp. xi. + 90.)

To anyone who is concerned with the problem of the helicopter this volume must be of considerable interest, if only because it is, so far as the writer is aware, the first technical book devoted entirely to the subject. Certain portions of the work have previously appeared as a technical supplement to "L'Aéronautique." The first part of the book is given up to the exposition of experimental results obtained with model airscrews and is of special value in so far as a wide range of V/nD, for positive and negative values, is covered, with the plane of rotation of the screw making various angles from $+90°$ to $-90°$ with the direction of the wind. At the same time one wishes that the experimental results had been presented more completely in tabular form and that M. Margoulis had extended his researches to cover a smaller pitch/diameter ratio. A chapter is devoted to the special study of screws working without forward motion. Here, quoting experiments of Durand and Lesley (U.S. Report No. 30), the author points the interesting result that "the thrust of a two-bladed screw, when at rest axially,* depends only on the diameter, the rotational speed, and the power expended, whatever may be the section, the blade width, the plan form, the pitch and its change along the radius."

In the second part of the book helicopter flight is considered and the author deals with the general case of a machine fitted with both sustaining and propulsive screws. In the case of vertical descent, with the motors running or shut off, there appear to be three possible conditions giving the same speed of descent, two of which are stable and one unstable. On the subject of free descent with engines shut off, the author does not seem very optimistic. Horizontal and oblique flight are also treated at length.

To English readers the greatest drawback is the frequent use of nomographs and Kith's logarithmic curves, the use of which is not so common here as in France.

The Internal Combustion Engine

By Harry R. Ricardo, B.A., A.M.I.C.E., M.I.A.E. Volume I. Slow Speed Engines. (Blackie and Son, Limited.)

As a rule, the more one studies the technical problems of aeronautics the more cautious one is likely to be in predicting the future. The amateur sees visions while the expert does the work. It is refreshing to find that Mr. Ricardo combines the enthusiasm of the amateur with the knowledge of the expert. "It is rather to the air that we must look for the influence that the internal combustion engine is destined to exert on the civilisation of the future. However great the progress in motor cars, in stationary engines and in marine engines of

* *Au point fixe.*

the internal combustion type, it is in connection with air locomotion that the internal combustion engine will probably play the most important rôle."

These words are taken from the introduction to the first volume of Mr. Ricardo's treatise on the internal combustion engine. The author is well known for his work on the high speed engine. He is one of a small band of engineers who, by their mechanical genius and scientific insight, have developed the high speed engine in this country to a degree unsurpassed, and perhaps unequalled, by any other nation. More than ordinary interest therefore attaches to this book in which Mr. Ricardo expounds and illustrates his beliefs and methods.

The first volume is devoted to the study of slow speed engines. The opening chapters deal with general thermodynamic considerations on accepted lines. Then follow some suggestive and original chapters on mechanical and volumetric efficiency and on general principles of design. The author's treatment of the problem of mechanical losses is particularly interesting; he rightly points out that in practice these losses are just as important, and perhaps more so, than thermal losses; yet far more scientific research has been directed to the improvement of thermodynamical than to that of mechanical efficiency. In the rest of the book examples of different types of slow speed engines are considered in detail, and thus serve to illustrate the general principles developed in the earlier chapters.

Most readers will agree with the author that the gas engine has seen its best days. In small sizes it cannot now compete with the electric motor supplied with current from a central station; and in large installations it has been beaten by the steam turbine. Not everyone will, however, share his views on the Diesel engine. His analysis of the technical advantages and disadvantages of this type of engine is clear and convincing; but when he compares the commercial value of Diesel engines, gas engines and steam turbines, we think he is too much influenced by pre-war conditions. Further, though the Diesel engine has not been developed to the same degree as the "petrol" engine since 1914, it has not stood still; and the author has perhaps hardly given sufficient consideration to recent developments. We should have liked, in particular, to have learnt his views on the proposed Diesel steam combinations, which utilise exhaust heat.

For those interested particularly in aircraft engines, this book will serve mainly as a general introduction to the second volume, dealing with high speed engines, to the appearance of which we shall look forward with great interest. There are comparatively few misprints in the first volume; its worst fault is the absence of an index. Even a list of illustrations would have helped; then perhaps we should not have spent so long in finding Fig. 15, owing to a misprint on p. 51. Perhaps a complete index to the two volumes will be given in the second volume. We hope so; if not, it will not be easy to devise a punishment to fit the crime. Something with boiling oil in it will certainly be necessary.

Who's Who in Engineering, 1923

Compendium Publishing Co., London.

The third edition of this reference book has been improved out of all knowledge compared with its predecessors, and now constitutes a very valuable contribution to the shelf containing those books which, though undoubtedly coming under Charles Lamb's stricture of being "*biblia abiblia*," are nevertheless a necessity for office purposes. The list of engineering institutions seems to be exceedingly complete, and is made more useful by the inclusion of such bodies in the British Dominions. Another feature which may be commended is the alphabetical list of abbreviations in use to denote the various grades of membership of technical bodies, as these are so frequently given incorrectly, even at times by the

individuals concerned. How often, for example, has one seen a member of the Institution of Civil Engineers described as "M.I.C.E."? The classified register of engineers appears to be tolerably representative so far as aeronautical engineering is concerned, though it is naturally not by any means exhaustive. To the ordinary person connected with aeronautics this reference book is probably more useful than the ordinary "Who's Who," and seems to be, on the whole, more complete than Whitaker's Almanack. In fact, if only one work of this sort is to be kept, this is probably the best for engineers.